walls of class-rooms. They are certainly to be compared with maps for the range and precision and correlation of parts with which they show their contents at a glance.

E. B. TYLOR

OUR BOOK SHELF

Aus der Urzeit. Bilder aus der Schöpfungsgeschichte, von Prof. Dr. Karl A. Zittel, in München. Mit 78 Halzschnitten. (München: Rudolph Oldenbourg, 1871-2.)

THIS is one of a series of popular works on Science entitled "Die Naturkräfte," that are being published at intervals by Herr Oldenbourg, of Munich. Prof. Zittel, in his preface to the present work, speaks of the vast influence which popular scientific literature is calculated to have upon the entire development of a people, and therefore insists on the great importance of diffusing, in an intelligible manner, among the people thoroughly correct notions of every science, instead of mincing down scientific truths until they lose all that is characteristic or informing. It is, perhaps, of far more importance that scientific books meant for the people should be as absolutely correct and as far advanced as it is possible to be, than those intended for scientific men themselves. The latter can discover and reject the false or imperfect; the former in their ignorance accept what is written as the truth, and the injury thus done is often serious in its consequences and may take a generation or longer to remedy. Popular scientific works, like school text-books of science, ought to be written only by those who are thoroughly masters of their subjects. The book before us seems to us to be in this respect satisfactory. In a series of chapters, each corresponding mainly with one of the great geological periods, the author endeavours to present a series of pictures of the gradual development of our earth, mainly with reference to the life which it supports. He seems to know his subject well in all its aspects, and presents in an interesting and intelligible way the latest results of geological research, with the conclusions derived therefrom by the most advanced thinkers. The illustrations are very good, and the work as a whole is a good specimen of a popular scientific treatise.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Remarkable Phenomena

IT may be within the memory of some of your readers that between the 15th and 20th August, 1868, a succession of waves reached Sydney, and were recorded by the self-registering tide-gauge. The average interval between the waves was about 25 minutes, and the greatest oscillation 34 inches, measuring from the crest of one wave to the hollow of the next. It was thought at the time that they were earthquake phenomena.

A similar visitation has just reached us, but it was not so marked in its character. The self-registering tide-gauge shows that the disturbance began during the afternoon of the 15th, and attained its maximum between 1 A.M. and 4 A.M. of the 17th, the greatest oscillation, amounting to 5 inches, occurred between 3.15 A.M. and 3.33 A.M. of the 17th, the average interval of the waves at this time was 25 minutes, but the average of 20 between 8 P.M. and 5.30 A.M. was 28 minutes. The waves cannot be traced beyond the 18th.

On the afternoon of the 16th we had a thunderstorm, during which the barometer was very unsteady, and the barograph sheets show some peculiar curves; strange to say, the average interval of the 5 most conspicuous of the barometer curves or waves between 5.40 P.M. and 7.30 P.M. is 25 minutes; the largest oscillation was 0·045 in. of mercury, equal to about 6 inches of water. Just before daylight on the morning of the 17th several fine meteors were seen to N.E., but the observer who reported them to me did not make notes of particulars. At Newcastle, which is a port 60 miles north of Sydney, I have another self-registering tide-gauge, which recorded a disturbance similar to the Sydney one; it began on the afternoon of the 15th and was greatest between 8 P.M. of 16th and 7 A.M. of 17th; the greatest oscillation, 9 inches, occurred between 12.15 A.M. and 12.30 A.M. of 17th, and the average interval of all the waves from 8 P.M. to 5.3 A.M. of 17th, amongst which are several that only occupied 5 minutes, and look like double oscillations, is 20 minutes.

Struck by the circumstances that both sets of waves, though separated by an interval of 5 years, occurred in August, I determined to examine all the tide-gauge sheets since 1866, when the instrument was set up, and was surprised to find a repetition of it every year, the amounts were too small individually to attract notice, but are nevertheless unmistakable, the periods are as follows:—

1866 August 9th to 10th, and again 15th to 21st.
1867 „ 5th „ 13th, very marked from 9 A.M to midnight of 12th.
1868 „ 15th „ 20th, remarkable (see beginning of this letter).
1869 „ 11th „ 17th.
1870 „ 12th „ 22nd, marked from 5 P.M. of 17th to 4 P.M. of 18th.
1871 „ 9th „ 10th and 20th to 21st.
1872 „ 10th „ 13th.
1873 „ 15th „ 18th, as recorded in this letter.

It is not easy to believe that earthquake phenomena will recur with such regularity, and we must seek another cause depending it would seem on the earth's annual motion, and to a certain extent affecting air and ocean alike.

It would be premature to express a decided opinion without further investigation, which I have not had time to make yet, but it seems very probable that the August meteor stream through or near which the earth passes about 10th August may be the cause. It will be observed that even in the few observations given above there are indications of a five-year period; for the double disturbance of 1866 is reproduced in 1871, and the great disturbance of 1868 is followed by a similar one in 1873.

Sydney Observatory, Aug. 23 H. C. RUSSELL

Periodicity of Rainfall

I DO not altogether agree with Governor Rawson when he says, in his interesting letter in NATURE, vol. viii. p. 245, that "the experience of Barbados is opposed to the theory broached by Mr. Meldrum and Mr. J. N. Lockyer." On the contrary, I rather think that Mr. Rawson's figures support the theory. He has taken 1846 and 1871 as middle maxima years (in my first paper I also took 1848), whereas 1849 and 1872 are probably more correct. Making this slight alteration, we get, according to Mr. Rawson's statistics:—

	Years.	Rain. In.	Sums. In.
Min.	1843	45·31	163·67
	1844	74·45	
	1845	43·91	
Max.	1848	63·77	184·42
	1849	52·77	
	1850	67·88	
Min.	1855	77·31	186·70
	1856	48·49	
	1857	60·90	

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	Years.	Rain. In.	Sums. In.
Max.	1859	56·22	187·95
	1860	57·91	
	1861	73·82	
Min.	1866	59·68	174·21
	1867	69·93	
	1868	44·60	
Max.	1871	41·46	154·85
	1872	48·39	
	1873	65·00	

Grouping the results we obtain :—

Rain in Max. Years.	Rain in Min. Years.
184·42	163·67
187·95	186·70
154·85	174·21
527·22	524·58

; showing an excess of 2·64 in. on the maximum side.

The quinquennial periods, as far as they admit of comparison, give also an excess in favour of the maxima years.

The heavy falls in 1844 and 1855, and the comparatively small fall in 1872, are apparently opposed to the theory; but it should be borne in mind that rainfall is greatly affected by local causes, and that to reveal the effects of a weaker but more general cause we must, as far as possible, eliminate chance, by comparing the total falls in maxima and minima periods. Tried by this preliminary test, the experience of Barbados can scarcely be said to be opposed to the theory.

My main object, however, is to draw attention to some discordances between Mr. Rawson's figures and those given by Mr. Symons in NATURE (vol. vii. p. 143); for until this disagreement be explained, there will be considerable uncertainty respecting the rainfall of Barbados. The following table will show where the two statements are at variance :—

	Years.	Rain. (Mr. Symons.)		Rain. (Mr. Rawson.)	
Min.	1843	45·3	163·7	45·31	163·79
	1844	74·5		74·45	
	1845	43·9		43·91	
Max.	1847	42·5	158·3	48·10	164·64
	1848	62·8		63·77	
	1849	53·0		52·77	
Min.	1855	73·5	170·7	77·31	186·70
	1856	46·4		48·49	
	1857	50·8		60·90	
Max.	1859	55·1	186·6	56·22	187·95
	1860	60·4		57·91	
	1861	71·1		73·82	

The greatest differences are in 1847, 1855, and 1857, and amount (for these three years alone) to 19·4 in.

It is worthy of remark that both statements show an excess on the side of the maxima years; Mr. Rawson's of 2·2 in., and Mr. Symons's of 10·5 in. But how did such great differences arise.

A remark made by Mr. Rawson may explain the matter. He says "the average of the island for twenty-five years, from 1847 to 1871, is 57·74 inches, based upon the mean of three stations in 1843, and increasing to 141 in 1871." Now it would be useful to know how the mean yearly rainfalls were determined. Is the fall given for 1844 (74·45 inches) a mean of the falls at three stations, and the fall for 1872 (48·39 inches) a mean of the falls at 141 stations? If so, and if the other yearly means were similarly obtained, Mr. Symons may not have taken the same number of stations as Mr. Rawson. Yearly means thus determined would not of course be comparable, for even in a small island the rainfall varies greatly according to locality. The rainfall in maxima and minima sunspot years cannot be fairly compared except by taking the same number of gauges and the same stations; and it is desirable that the falls in the intervening years should be given.

"Assuming that sunspots affect all parts of the globe *equally*, and that periodicity prevails in all *alike*," Mr. Rawson, with the above experience of Barbados before him, is "led to the conclusion that it was 'chance alone' that led to the coincidences noticed by Mr. Symons." Now the theory makes neither of these assumptions. It assumes that there is a sunspot periodicity; that this periodicity implies a secular variation of solar heat and radiation; that, therefore, there is a corresponding periodicity of temperature, wind, and rain on our earth; but that, from various counteracting causes, the observations at some stations will not show a periodicity, while those at a large majority of stations, and a mean of all the observations, will do so. In short, with respect to rain, the theory assumes that the annual fall over the globe is subject to a variation, corresponding with the sunspot variation, but that from disturbing influences, local exceptions must be expected. Granting, therefore, that the rainfall of Barbados is opposed to the theory, I do not think it follows that the favourable experience of the British Isles must be owing to chance alone; for that experience is what theory leads us to expect, and it is much more extensive both as to time and space than the experience of Barbados. If England and Barbados were the whole globe, the theory would be wellnigh proved, as far as observation goes; for, according to Mr. Symons's Table I, there was not, from 1815 to 1864, a single exception to the rule that more rain falls in the maxima years; and if we take the aggregate falls for England and Barbados from 1843 to 1873, it will be found that there was a large excess on the maximum side.

I have now examined 93 rainfall tables from various parts of the globe. They are all I have as yet been able to procure, and they have been published *in extenso*, so that the evidence they afford may be scrutinised. That evidence is such that if no rain at all had fallen at Barbados in the nine principal maxima years since 1843, and the rainfall in the nine minima years were to be put in the other scale of the balance, there would still be a large surplus in favour of the theory. Up to the present time the more numerous the observations, the stronger the evidence. Still I shall be prepared to abandon the theory whenever a preponderance of undoubted facts may be brought against it. But I see no prospect of this, for the rainfalls of England, Scotland, the Continent of Europe, India, Africa, America, and Australia, as far as they have yet been examined, sustain the theory.

C. MELDRUM

Mauritius, Sept. 15

Dr. Sanderson's Experiments and Archebiosis

DR. SANDERSON has strangely misunderstood the wording of my letter which appeared in NATURE on the 9th inst. Any one may see that I did not challenge him to "deal" with my main proposition "that Bacteria are capable of arising in fluids independently of living reproductive or germinal particles." That position was merely alluded to by me in order to show the relevancy of the question which I asked Dr. Sanderson: and the question itself was—"Whether he still believes that Bacteria are killed by a temperature of 100° C. in fluids; and if not upon what grounds he has changed his opinion?"

Whilst tacitly declining to answer this question, Dr. Sanderson now says, "I hope that Dr. Bastian will allow me to decline to enter on the general question." But it is precisely because Dr. Sanderson has distinctly expressed himself upon the general question both at the late meeting of the British Association and in your columns (NATURE, vol. viii. p. 181), that I feel he may, both from a moral and from a scientific point of view, be called upon to reply to the question above quoted.

The need that Dr. Sanderson should express the grounds of his opinion concerning the death point of Bacteria in heated fluids is further shown by Mr. Ray Lankester's communication in last week's NATURE, in which he says, "Dr. Sanderson does not believe that there is a definite relation between the precise temperature to which the infusion is exposed and the destruction of Bacterian contamination." Now if this is really Dr. Sanderson's present opinion, it may not inappropriately be asked whether it is an opinion based upon definite evidence or whether it is a mere surmise? I say the question is not inappropriate because, as Dr. Sanderson will recollect, I have heard from his own lips, since his return from Bradford, that he has made no definite observations upon the subject, and that he is quite unprepared to question the truth of the experimental evidence which I have recently brought forward (Proceed. of Royal Society, Nos. 143 and 145) showing that Bacteria are killed in fluids which have been raised for five minutes to a temperature of 60° C. (140° F.)*

Dr. Sanderson previously supposed that Bacteria were incapable of appearing and rapidly multiplying in certain fluids

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* I should have hesitated about referring to what has passed in conversations between Dr. Sanderson and myself, if he had not set the example both in your columns (NATURE, vol. viii. p. 181) and in a discussion at one of the meetings of the Royal Society.

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